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THE ASTRONOMICAL THEORY OF THE ICE AGE.

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For THE MESSENGER.

This subject has perhaps sufficient interest for astronomers to warrant the insertion in THE SIDEREAL MESSENGER of the following remarks partly suggested by Professor McFarland's note in the March number. At the outset I wish to say that I do not think the phrase, "Astronomical Theory," should be limited to that of Dr. Croll. Besides including the counter-theory of Mr. J. J. Murphy, that phrase is also wide enough to include the theory which makes the sun a variable star of long period. This period I suspect will prove to be much less than 200,000 years. The Ice Age may not improbably have occurred not more than 20,000 years ago, in which case, of course, both Croll and Murphy would be put out of court. The best test of this is the amount of post-glacial erosion, of which the Americans have a magnificent example in the falls of Niagara.

I think, however, that neither Dr. Croll nor Mr. Murphy have ever stated the problem in its general form — much less attempted to solve it. It is admitted that during the period when the earth's orbit was most eccentric the total heat received from the sun in each year was not diminished, but in fact slightly increased. And (subject to the qualification to be made hereafter) the same remark is true of every locality on the earth's surface. The problem therefore is: Supposing the total heat to be constant, what distribution of it is most favorable to the formation of a permanent snow cap or ice cap? Without stating the problem generally, Dr. Croll virtually replies: The best arrangement is that which gives an under-supply of heat for the longest time and an over-supply for the shortest; while Mr. Murphy replies: The best arrange-

ment is that which gives the longest duration to the over-supply, and the shortest to the under-supply. (The words "over-supply" and "under-supply" refer to a supply in excess or defect of the mean.) It is essential to both theories that time should enter as a factor into the problem; and in one sense it is obvious that it does so. What I intended to convey by my former remarks in THE SIDEREAL MESSENGER (and I think I expressed my intention with sufficient clearness), was that time is not an *independent* factor in cases where the total annual heat is constant. If q be the amount of heat received in the unit of time and Q the amount received in the year, then $q_1 t_1 = \frac{1}{2} Q$, and $q_2 t_2 = \frac{1}{2} Q$ where q_1, q_2 are the mean heat of summer and winter respectively, and t_1, t_2, the lengths of the summer and winter respectively; and the quantities Q and $t_1 + t_2$ are both constant according to Croll. Time is thus a factor which is invariably associated with another factor that varies inversely as the time, and the element that we have really to consider is the product of the two factors, which is constant. If Professor McFarland places a block of ice at the temperature of 32° *F*, on a surface whose temperature is kept constant at 33° *F*, he will find that the amount of ice melted at the end of an hour is exactly equal to what would be melted in a minute if the surface temperature was kept constant at 92° *F*.

The problem of the formation or melting of a snow cap or ice cap is not statical but dynamical, and the quantity of heat, not the temperature, is primarily to be looked to. If we desired that a given quantity of heat should melt the maximum amount of ice, we would endeavor as far as possible to keep it from raising the temperature of the air or other surrounding objects. But Dr. Croll and his followers seem to me to forget the equivalence of heat and work, owing to which heat must always be employed either in raising the temperature, in melting ice, or in doing both; the same quantity of heat always performing the same amount of work. Dr. Croll, for instance, insists on Tyndall's statement that a joint of meat could be roasted while the surrounding air was as cold as ice; but did Tyndall ever say that a joint of meat could be roasted where a block of ice could not be melted? Or that rays powerful enough to

melt pitch would fail to produce any effect upon ice? Croll is no doubt right in saying that if India was covered with an ice cap the summers would be cold. But why? Simply because most of the summer heat would be employed in melting the snow and ice. The low temperature would be due to the rapidity of the melting. When it is once admitted that the total quantity of heat received from the sun in each year at the period of greatest eccentricity is as great (or rather a little greater) than at present the question arises: How is it possible that this equal quantity of heat proves insufficient either to melt the winter accumulation of snow or to raise the summer temperature above the present level? —for Dr. Croll and his followers will not allow it to have either effect. The total effect in the two departments must be the same as before, on the principle of the conservation of energy, but Dr. Croll apparently ascribes to it a diminished effect in each department. Many of Dr. Croll's arguments on this point may be retorted. Is the air clear and dry? The result is favorable to the theory, according to Croll, because the temperature of the air is not raised. Is the air moist and foggy? The result is equally favorable, in his opinion, because the incident heat is absorbed by the air and does not reach the snow and ice. But in the former case it might be replied that the incident heat produces its maximum effect in melting because none of it is exhausted in raising the temperature of the air; and in the latter case, that since the absorbed heat raises the temperature of the air, the cold will not be as great as Dr. Croll supposes. But any view which takes in only one-half of the subject is misleading, whether it is Dr. Croll's half or the other half.

I do not deny that there may be special cases in which Dr. Croll is correct, as also special cases in which Mr. Murphy is so. What I deny is that either of them has stated or solved the problem in its general form. If we take a locality well supplied with moisture, in which the annual heat, if uniformly distributed, would keep the temperature constant at 28° or 30° *F*, an uniform distribution of this heat would be favorable to a permanent snow cap. The snow could never be melted though it might be slowly evaporated, and snow usually falls in the greatest profusion when the temperature is not much below freezing point. An unequal dis-

tribution of heat would be necessary to clear off the snow cap during any portion of the year, however short, and I think there can be little doubt that a sufficiently unequal distribution would enable the ground to be cleared of snow for a few days or weeks in summer. Indeed, if the winter was very short but intensely cold, such a summer clearance would be almost certain. On the other hand if the supply of heat was sufficient to produce an uniform temperature of 34° *F*, I doubt whether a permanent snow cap could be produced; but without an unequal distribution of temperature there could not be even a temporary one.

There are, I think, but two of Dr. Croll's arguments which are worthy of consideration by scientific men, for his talk about long cold winters and short hot summers is evidently intended for ordinary readers only, who are likely to overlook the principle of compensation by which length is always attended with feebleness, and shortness with intensity. These arguments are: 1. Snow and ice are good reflectors of heat, and, therefore, when a snow cap is once formed the incident heat is practically reduced, inasmuch as a large proportion of it is reflected back into space without doing any terrestrial work. 2. The northern and southern regions at present receive large quantities of heat from the equatorial regions by means of ocean currents and air currents (Croll seems to regard the latter as unimportant); but when northern or southern ice caps were formed these currents would be diverted, and the glaciated regions would thus be deprived of a considerable part of the heat which they now receive.

To commence with the latter argument, it presupposes the validity of some of the others; for the cause of the diversion of the ocean currents, according to Croll, is the increasing coldness of the region from which they are diverted. Thus suppose the northern region to be the colder one, the ocean currents would be diverted towards the south, and thus intensify the northern cold by diminishing the supply of heat from the equator. It is evident that this reasoning would not hold good if the main agent in producing ocean currents is the difference of temperature between their extremities. The cause of ocean currents is still under debate, but for the present purpose I will assume it to be what Dr. Croll sup-

poses. The winter being coldest when the eccentricity is greatest, and the earth is at aphelion in winter there will therefore at this period be a southern diversion of the ocean currents during the winter. But what of the ensuing summer? If it is likewise hotter (as it ought to be from the greater quantity of heat received in the same time), there will be a counter diversion of the ocean currents to the north which might not impossibly break up snow caps and ice caps that remained undisturbed so long as the ocean currents oscillated within narrower limits. At all events the summer heat will be as much increased by the northern diversion as the winter cold by the southern diversion, and the prospects of a permanent snow cap increasing from year to year are in no way improved by the change.

Assuming the quantity of heat received in the year to be unaltered, the mean temperature of the year will not be affected by the formation of snow and ice so long as the summer heat suffices to melt the winter accumulation. The fall of temperature is as much checked by the formation as the rise is checked by the melting. But in order that the accumulation of snow and ice should increase, the mean temperature of the year must be raised; for the fall of temperature is more checked by the formation than the subsequent rise is checked by the melting. And if the accumulation of snow and ice diminishes during the year, the temperature must be below the mean; for the rise of temperature is more checked by the melting than the fall is checked by the formation. Consequently if it is the mean temperature of the year that fixes the mean position of the ocean currents, this mean temperature will be above the average when the snow and ice are increasing, and the mean position of the currents will be farther north than before. I do not, of course, affirm that this was actually the case when the snow caps and ice caps were forming. I only say that it is what must have occurred if Dr. Croll is right in maintaining that there was no decrease in the total heat received in each year during the formation. If so, the mean temperature must have increased during the formation of the ice caps and diminished during their dissipation. Dr. Croll will perhaps find a confirmation of this view in the fact that (according to Mr. Ferrel) the mean temperature of the Antarctic regions, where there is

at present a winter aphelion and a partial glaciation, is higher than that of the Arctic regions at equal latitudes—a result which Dr. Croll accepts.

It only remains to consider the loss of heat by reflection from the surface of snow and ice. I doubt whether this is considerably greater than the reflection from the same surfaces when free from snow and ice. Observations and statistics on this subject are highly desirable, and in fact they seem to me to be the only way in which Dr. Croll's theory can be proved. The differences between the reflecting powers of different substances diminish as the angle of incidence increases, and we are dealing with regions where the angle of incidence is always large. The surfaces of snow and ice, too, often become covered with impurities which seriously diminish their reflecting power. The question, however, is one which I leave experimentalists to deal with, merely calling attention to it as the turning point of the theory. On other points it may be said that Dr. Croll's theory would work admirably if more heat was absorbed in melting snow and ice than is given out in forming them; but the equivalence of these two quantities, together with the constancy of the total annual heat, seems to me fatal to it. For those who desire to pursue the subject I desire to refer to Dr. Haughton's researches on radiant heat recently published by the Royal Irish Academy. Dr. Haughton's results, though not arrived at with any controversial object, appear to me to be very unfavorable to the views of Dr. Croll.

ASTRONOMY IN THE UNITED STATES.*†

T. H. SAFFORD, PH. D.

In the quarter century which elapsed between the first beginning (1836) of our Observatory and the outbreak of the civil war, American astronomy had made great progress. The work of an astronomer involves certain professional habits of care and accuracy, whether he be chiefly an observer or a calculator; and an abstract mathematician does not always make the best practical astronomer, for the latter must attend to certain every-day matters which the former some-

* A discourse read June 25, 1888, to commemorate the fiftieth anniversary of the dedication of the Hopkins Observatory at Williams College, Williamstown, Mass.

† Continued from No. 72, p. 77.

times neglects. The best cure for absent-mindedness and day-dreaming which I know, is to observe star-transits; for the stais are extremely punctual; if the observer wanders off into regions of abstract thought, the star will not wait for him, it is always in its proper place at the exact second.

Then, there are mechanical operations to be performed which sometimes involve a good deal of manual labor; and the man who has to do this must be skilled in the science; even the subordinates in an Observatory need education; they must have a good deal of mathematics at their finger's ends.

One great advance in mathematics during the present century is the theory of the errors of observation which we owe to Gauss, and which shows us how to distinguish between good observations and bad, and even between blunders and the necessary imperfections of our senses.

By 1861 this country had acquired what I may call a school of astronomers. That is, there were many observatories,—public and private,—with far too little money for their maintenance in regular activity, it is true, but with here and there an observer or calculator who knew how to use the instruments and the results of observation. Any young man who, like Rittenhouse or Bowditch, felt himself impelled to study astronomy, could find instructors, and after sufficient training could usually get remunerative employment. He could also gain the ear of a public interested in such things; by the better newspapers, if what he had to say was of a popular character; or by the scientific journals of America or Europe, if he had something new and original for the specialists. There was also an astronomical journal of much merit, published by the zeal and munificence of its editor, Dr. Gould.

Some of the early achievements of our astronomers have been of permanent use to the science. Of these the most important are the two connected inventions,—the chronograph, and the telegraphic method of determining longitude.

The chronograph is the instrument used in the "American Method" of observing transits. It is practically a telegraphic ink-writer, or other register; connected with a clock, it marks the seconds on a sheet or tape of paper; and the observer, who has to find the exact time of any astronomical

phenomenon has simply to press a telegraph key near his instrument, and the time is recorded. Similar instruments have since been used to measure the speed of thought and compare one man with another as to quickness of apprehension, or willing; so that astronomical methods have thus been introduced into psychology. In the old way of taking transits, the observer, while looking through the telescope, was obliged to count his time, second by second; to do this without mistake, and write down the small fractions of a second, is much more difficult than to observe by the American method.

Very soon after Morse's inventions, and the establishment of a few telegraph lines, Walker, Loomis, and other American astronomers, used them to send time from one Observatory to another. We can readily see that by the earth's steady motion on its axis, difference of time is equivalent to difference of longitude; twenty-four hours correspond to the whole circuit of the earth, and every hour to fifteen degrees. All our railways are now run by Greenwich time, with a change in the whole hours only; thus seven o'clock of railway time here is simply twelve o'clock at Greenwich; and our trains are run on the time of the seventy-fifth meridian.

Before this system could be inaugurated, our astronomers must find out how their Observatories were situated with respect to the Greenwich meridian; or at any rate to *some* meridian. The telegraph was, as I have said, first employed in America for this purpose, after many trials of other methods had been made, with partial success. Before the ocean cable was laid, the position of Harvard Observatory from Greenwich had been determined with great care by the Bonds; their method of exchanging times was to send a great many ships' chronometers backwards and forwards between England and America.

Our astronomical progress had been most considerable in those branches which are of practical importance; but yet there were those who gladly took hold of more ideal problems. The American mind is peculiar; partly from heredity, partly, I suppose, as influenced by the greater command of circumstances possible in a free country. The American is ambitious in intellectual things, if once his interest is aroused; and he frequently cannot reconcile himself to take a second place.

A striking example of this quality is seen in the life of Alvan Clark, the great optician, who has lately gone from us at a ripe old age. In my boyhood I met him, then a modest portrait painter of middle age, who had begun to interest himself in the making of object glasses, and to hope that he could by-and-by compete with the German opticians in telescopes of moderate dimensions.

One of his early object glasses, of half the diameter of the Cambridge telescope, is that belonging to the equatorial of the Williams College Observatory, given by that constant friend of the college, Amos Lawrence. Mr. Clark did not then construct the machinery of entire telescopes, and the mounting is by an inferior mechanic, and not very good.

It was not long before Mr. Clark's reputation increased, and he received some orders from England. His acquaintance with the English amateur astronomer Dawes gave him opportunity to learn what a very sharp-sighted, careful observer desired in his object glass,—for Mr. Dawes was extremely critical,—and he was finally able to surpass even the German makers in the precision of the images seen in his telescopes. At this point he was assisted by liberal capitalists to set up a larger establishment, and, with his sons, to enter upon telescope-making on a greater scale.

His first great telescope, belonging to the Chicago Observatory, was completed in 1865; the object glass, larger than any then existing, had been made several years before. From that date until his death in 1887, he was actively at work, though already an old man; and my last visit to him, two years ago, was made in his workshop, where he was busy on the greatest object glass now existing, one of more than twice the diameter of the Harvard telescopes. This glass, that of the Lick Observatory, in California, was preceded by one which gave him great triumph. He had displaced his competitors' instruments in America, wherever increased dimensions were called for; but the great Imperial Observatory of Russia, at Pulcova, near St. Petersburg, was in the market for such an instrument.

The Struves, father, sons and grandsons, have long been known as among the most careful observers. But the present head of the family became convinced that in his own special line of work he needed a more powerful telescope;

and what he learned of the performance of the Clarks' glasses led him to give them the order for the optical part of the instrument. The mechanical portion was made in Hamburg, by the Repsolds, who are the greatest mechanicians of the world, the makers of our fine meridian circle.

The last twenty-five years have brought much material advancement to the science in this country. It is hardly possible to go deeply into it; in many respects it is a repetition of the earlier history. Observatories have been founded in new places, sometimes with means for their maintainance, at other times without. Some of the older ones have received large accessions of invested funds, and have thus been enabled to do more; this is notably the case at the Harvard Observatory, which has been given the handsome fortunes of Robert Treat Paine, the amateur observer of the earlier time, and of Uriah Boyden, an inventor of turbine wheels, who had been greatly aided by Benjamin Peirce's knowledge of the higher mathematics. Boyden's bequest requires the establishment of a mountain Observatory; his trustees have placed it under the Harvard management, so that the mountain observations will be calculated at Cambridge. The ability and success of our younger astronomers in handling deep and difficult problems have been proved entirely adequate; I think we have never lacked the men, but it is only lately that they have found education and encouragement.

Many able astronomers, too, have come to us from foreign countries, among whom is our Nestor, Dr. Peters, who is with us on this occasion. I dare not attempt to say how many small planets he has discovered within the last quarter of a century, but his other work has been enough by itself to give his Observatory a high reputation both at home and abroad.

It is pretty plain that the public mind has changed its attitude towards astronomy. We now find more general intelligence on the subject; more disposition to believe in astronomers; more encouragement to those of them who are still struggling with difficult problems; more pride in their achievements; European scientists now come occasionally to see what is doing here; the profession of an astronomer is a recognized career.

What then shall be the future of our science in this country?

Two things are plain: first, that the great benefactions to colleges help all sciences; and second, that original investigation is much more prominent as a feature of college work than ever before.

We have giant telescopes enough in this country; Alvan Clark's sons will doubtless keep up the supply of large instruments; but we need to look at the science a little more deeply on the intellectual side. I would remind you that our college studies are largely traditional; that astronomy, along with geometry and music, was one of the studies of the old Quadrivium, and that perhaps a recasting of our courses may be possible and beneficial.

I am a believer, as are other college instructors of some eminence, in the disciplinary value of astronomy as an independent study. The mathematics have their value, and a very high one it is; but the lower mathematics, especially arithmetic, have been overdone in a certain direction; I mean that of riddles, puzzles,—brain-spinning, as the Germans call it. While our boys and girls are given problems to solve which quite exceed their thinking powers—I don't suppose I could ever have gone successfully through Greenleaf's National Arithmetic till I had graduated from college—their minds are quite undeveloped in the power of observation, and they are often imperfectly trained in the four ground rules, especially in decimal fractions. So far as my own experience goes, the best mathematical training is that which deals with tangible objects; the abstractions should have a sensible basis.

I would then have the observation of the common phenomena of nature accompany the study of arithmetic and geometry in the common schools. The pupils should learn to watch the barometer and the thermometer, sunrise and sunset, the phases of the moon, the motions of clouds; they should know the pole-star, Ursa Major, Orion, the Pleiades, Leo, the Scorpion; should learn to distinguish between the stars and the planets, to watch for the aurora borealis, to note the colors of the rainbow. The high school, or college preparatory school, should always have its telescope, and some simple means of accurately keeping time; a few no-

tions of scientific astronomy should not fail to be inculcated.

In college, the professor of astronomy should have time and opportunity to interest even the Freshmen in his study; I do not mean that he should give them formal teaching, this may well be reserved for later years. But he should have a variety of instruments, some of the inexpensive kind made now-a-days, for gazing purposes; or old telescopes, out of fashion for observation, so that one or another of the students could watch the heavens for himself. Informal instruction may always well precede the more formal; and occasional observatory evenings with interesting objects could be arranged, so that a good part of our classes might enter on the regular study of the science with some distinct notions.

To introduce the work I have described into our common schools will take a long time, perhaps a generation. But it seems a waste, when we open any mathematical school-book and find in it so much that refers to a merely imaginary world; and then to hear from business men and college professors that all this training leads to nothing definite; and, when the young men are nearly through their college course, to find them unable to tell the points of the compass in a strange place, or even in their own college town; or not aware that when the moon is full it rises very nearly at sunset.

The colleges have begun to do their part in teaching the teachers. Courses in practical astronomy are now given in many institutions; the instruments can be used, and the results of observation calculated, by the few who elect this subject.

It is more difficult than it ought to be to go very far in these studies, because the habit of applying the earlier mathematics to tangible objects is unformed. The young man is at first confused when you tell him that he must measure the sides or angles of his spherical triangle. He has always thought a spherical triangle to be an abstraction. Moreover, he cannot always handle even his arithmetic with facility; and certainly, again and again I have found mistakes in difficult calculations, which the student himself could not detect, to lie in carrying wrongly in addition or subtraction.

A return to nature in our whole method of education—even

in the elementary teaching of Greek, that bugbear to some so-called educators—is now actually going on; the next generation will reap the benefit. Could I have learned Greek and Latin as my colleagues are now teaching them, I should have had many more interested hours; and other subjects, history, biology, physics, are now taught by better methods.

The college Observatory of the future, in this country should contain a good many moderate instruments; none very gigantic, but some of that handy size which is best adapted to advance the science. A giant instrument renders the observer helpless, if he has not a file of soldiers, or other servants, to help him move the machinery; one which he can just conveniently handle leaves him free to work, and always shows him objects enough to observe. If he have a trained eye, that in itself is equivalent to an increase in the size of his instrument.

I hold that in a strong college, independent work, to advance the science, should be going on; the student will be benefited by the closer contact with realities which is thus gained. Moreover the teachers themselves can better be kept from rusting, or falling into a treadmill round; they can be in the current of scientific thinking, even if their problems be modest. But along with the best instruments to show the refinements possible, there should be samples of simpler and cheaper ones, partly for the independent work of the pupils, partly for the exploration of the heavens to gain immediate communion with nature in its grander aspects, partly to show the future teachers what their schools can afford. The general framework of mathematical training will gain largely, the more it is connected with modern scientific applications, and thus the more closely it is conjoined with nature.

I may be permitted here some quotations from a modest but deep thinker, whose little work Emerson recommended Carlyle to read, the late Sampson Reed, of Boston. The book is called "Observations on the Growth of the Mind."

Mr. Reed says: "If it were desired to make an individual acquainted with one of the abstract sciences, this might best be effected by leading him gradually to whatever conduced to the growth of those powers on which a knowledge of these sciences depends; by cultivating a principle of dependence on

the Divine Being, a purity and chastity of the affections, which will produce a tranquil condition, of all things the most favorable to clear perceptions; by leading him to an habitual observation of the relation of things, and to such continued exertion of the understanding, as, calling into use its full powers without inducing fatigue, may impart the strength of the laborer without the degradation of the slave; in a word, by forming a penetrating mathematical mind rather than by communicating mathematical information. The whole character and complexion of the mind will thus be gradually changed, till at last it will become (chemically speaking) in its very nature an active solvent of these subjects." This return to nature, in our teaching, was thus eloquently recommended in 1826; it is gradually becoming accomplished in the scientific studies of our better colleges.

Astronomy deals with immensity of space; who can conceive the enormous distances at which the stars are from us? The nearest one is forty millions of millions of miles from us, or nearly that; the light is six or seven years in coming from it. The little star which was seen to blaze out in 1866 may really have burst into flame before the discovery of America by Columbus; the dim cloudy spots which we see in so many places, and are tempted to call world-stuff (nebulous matter is the usual name), are so far that no human mind can do more than guess their distance.

It will be well, I think, if we can interest our pupils—and I do not mean college students alone—in the contemplation of the heavens as well as in the scientific study of their motions and phenomena; if the telescope can become an indispensable piece of apparatus in the highest school of any locality, even a village. This study should go hand in hand with the ordinary common observations of the nearer things around us.

But looking back over the last half century of scientific progress in America, we have every reason to be hopeful for the future. America is the great republic; every man is born equal to every other; if the bricklayer's son exhibits the genius of the century in mathematics he needs no petty Grand Duke's favor, but will be recognized and helped by his fellow citizens and the organizations for study which have grown up during our few centuries; while the experience of

the past fifty years has shown that these organizations will have an unexampled growth in the next fifty years so far as predictions in any human affairs are possible. American astronomers and American instrument makers—few indeed half a century ago—are now known by reputation and respected in the whole civilized world.

The first permanent American Observatory is still standing, to show by its modest dimensions how great a growth has been possible in half a century.

THE DOUBLE STAR, ε HYDRAE.

S. W. BURNHAM.

For The Messenger.

In measuring this pair (Σ 1273) at Pulkowa in 1860, OΣ suspected an elongation in the principal star in a vertical direction; and again in 1864, by an entirely independent observation, noted an apparent elongation in the direction of 190°. With this in mind I examined it in 1877–8 with the Chicago 18½-inch on several occasions, and always found the larger star round. It was also observed by Holden and Hall with the Washington 26-inch in 1875, and a new companion of the 14th magnitude discovered at a distance of 20″ from A. The Struve companion was measured by Hall in 1878, 1880 and 1883; and while it does not appear that the Washington observer specially looked for the suspected star, doubtless any irregularity in the figure of the bright star would have been noticed. In April, 1888, Schiaparelli, with the 18-inch refractor at Milan, found it plainly elongated, and measured it with that instrument on six nights. Last fall I turned the 36-inch on it, and at once saw the close pair, and measured it with a power (about 3300) which fairly separated the components. Subsequently it was observed again with the same instrument. The two sets of measures are as follows:

1888.28	P = 142°.0	D = 0″.21	4 . . 5.5	Sp	6n
1889.04	154 .4	0 .26	4 . . 6	β	2n

It is quite certain that this will prove to be a physical system, and perhaps one in rapid motion. It must be at all

times a difficult object, and will require a large aperture to properly measure it. The physical relation of the Struve component has long been established, although the movement is comparatively slow. The change from 1825 to 1889 amounts to about 33°.

The observations do not cover a sufficient period to show whether or not the distant companion is a member of this system. The following are all the measures:

1878.33	P = 193°.9	D = 19″.78	. . 14	Hl 1n
1878.60	192 .0	20 .05	. . 12–13	β 2n
1889.15	193 .5	19 .71	. . 13	β 1n

Altogether this must be considered as one of the most interesting star systems, and worthy of the attention of observers having telescopes of sufficient power to satisfactorily show the new stars.

Lick Observatory, March 14, 1889.

METEOR COMETS.

W. H. S. MONCK, DUBLIN, IRELAND.

For The Messenger.

That comets connected with any well sustained meteor shower must be periodical, seems pretty evident, and that there have been previous visits may be regarded as certain if the shower can be traced in the past. Only four comets have hitherto been connected with meteor showers with any degree of certainty. Of these four the history of Biela's comet need not be recapitulated. The period of the Leonid comet has been fixed with reasonable certainty at between thirty-three and thirty-four years, and it has been shown to be highly probable that the comet of 1366 was a previous appearance of this comet. The shower no doubt existed before 1366 and earlier comets have been identified with this meteor comet on probable grounds.

The question therefore whether any earlier appearances of the Perseid or Lyraid comets can be traced, and consequently their periods and those of the corresponding meteor showers fixed, is therefore of considerable interest. Both comets were fairly bright on the occasion of their last visits and are not therefore likely to have escaped observation on all previ-

ous occasions. The earlier comets, however, have been but roughly observed, and we cannot hope to discover anything like exact accordance between the elements of the meteor comets in question and their predecessors. Considerable differences exist between the elements of Halley's comet and those computed for earlier comets which were almost certainly identical with Halley's; and differences also exist between the elements of the Leonid comet of 1866 and those of the comet of 1366 with which it is supposed to be identical. A general resemblance in the elements and near coincidence of period is all that can be looked for.

The period of the Perseid comet (Comet III of 1862) has been computed at 123 or 124 years. Adopting this period I do not find any preceding comet which resembles it, but by increasing the assumed period to 131.3 years or thereabouts there is a fair general resemblance between the following three comets:

	π	Ω	i	q	μ
	° ′	° ′	° ′		
1337	2 20	93 1	40 28	0.828	—
1468	356 3	61 5	44 19	0.853	—
1862 III	344 41	137 26	66 25	0.963	—

Alternative orbits have been computed for the first and second of these comets which in some respects agree better with the third than those which I have selected. The times assigned for the respective perihelion passages are June 1337, October 1468, and August 1862, so that the interval between the second and third is almost exactly three times that between the first and second; but on the assumption of identity I find no trace of the returns of the comet in 1600 and 1731. Four more periods of this comet (if my conjecture is correct) would lead back to the year A. D. 813, when a comet appeared which seems to have exhibited considerable traces of disruption during its visit. Possibly this disruption may have been the origin of the Perseid meteors.

The Lyraid comet (Comet I 1861) was computed to have a period of about 415 years. The meteors (as might be expected from the length of the period) do not appear to be distributed over the entire orbit, and have already thinned out to a large extent. A great display of meteors is recorded on the 4th of April, A. D. 1095, which (allowing for precession) were very probably Lyraids. The comet must, in

this case, have returned in or about the year 1095, and the orbit computed for the comet of A. D. 1097 is in many respects similar, save that Burckhardt makes the *descending* node of this comet nearly coincide with the *ascending* node of the Lyraid comet. I would, therefore, suggest that this comet has very probably a period of 382 or 383 years instead of 415 as computed, and it appears from the *Chronica Bossiana* that a great comet appeared in September 1478, which may possibly have been an intervening return.

I may perhaps note that the perihelion distances of all four known meteor comets lie between 0.9 and 1.0. Making allowance for the roughness of early observations the comets of 1097, 1337 and 1468 agree fairly in this respect.

THE RELATIVE TIME OF ROTATION OF ANY COSMIC BODY A FUNCTION OF ITS RELATIVE DENSITY.

SEVERINUS J. CORRIGAN.

For THE MESSENGER.

In that portion of my paper on the "Effects of Rotation," published in No. 72 of THE SIDEREAL MESSENGER, I advanced the hypothesis that the axial rotation of any member of the solar system is due to an original orbital motion of the individual particles of which the body is composed, around a common "center of gravity," or nucleus, and that the angular velocity of rotation must depend upon both the original orbital velocity, and the degree of freedom possessed by each particle, whereby it can move among the surrounding matter. It is obvious that the degree of freedom must depend upon the number of resisting particles encountered by the moving element in its passage through any given space, and since the number depends upon the density, it is plain that the "time of rotation" must be some function of the latter. As evidence tending to establish the truth of this hypothesis, there was tabulated on page 58 of the above named number, among other quantities, the rotation period of each one of the eight principal planets, in terms of that of the earth, and also the square root of the density of each planet relative to that of our globe.

For the sake of convenience these quantities are again set forth in the following tabulation:

Planet.	1	2	3	Planet.	1	2	3
Mercury............	1.00	1.06	—0.06	Jupiter...............	0.41	0.49	—0.08
Venus................	0.98	1.02	—0.04	Saturn...............	0.44	0.36	+0.08
Earth................	1.00	1.00	0.00	Uranus..............	0.40	0.41	—0.01
Mars..................	1.03	0.97	+0.06	Neptune......	Unknown.	0.40	....

In the above table, column 1 contains the relative rotation periods, column 2 the square roots of the relative densities, and column three the differences between the two taken in the sense 1—2. While the relation between the quantities above set forth is remarkable, yet since the agreement between the rotation periods and the square roots of the densities is, in any case, only approximate, it furnishes only *presumptive* evidence in favor of the hypothesis. To render this relation conclusive proof, it must be shown by a rigorous demonstration that it is not a mere coincidence, as many readers may infer, but the result of an indubitable law of "celestial mechanics." I shall now endeavor to prove the existence of such a law. The proposition to be demonstrated may be stated as follows:

The relative "time of rotation" of any cosmic body formed by the congregation of particles originally independent, moving around a common "centre of gravity," and subject to only the force of gravity directed toward that centre, must be equal to the square root of the relative density of the body.

If we regard any cosmic body as an aggregation of particles endowed with orbital motion, and the axial rotation as a derivative of this motion, the demonstration can be effected most readily by considering the circumstances of motion of any element at the equatorial circumference of the body. Let A represent the angular velocity of rotation of this element, T its "time of rotation," and v its orbital velocity; furthermore, let M denote the mass, D the density, and k the unit of attraction due to the mass, of the body to which the element belongs.

It is to be *distinctly* understood that all of the above named quantities are to be regarded as only *relative*, the corresponding quantities pertaining to the earth being taken as the respective units.

Now, it is evident that if the particle were to meet with no resistance, its angular velocity would be equal to the orbital velocity, and that this relation would be expressed by the equation,

$$A = v; \quad (1)$$

but, since there must be a resistance due to the impeding or retarding action of surrounding matter, which indefinite resistance will here be denoted by R, the angular velocity will also be *inversely* porportional to R, and we will have the equation $A = \frac{v}{R}$, or, since the "time of rotation" is the reciprocal of the angular velocity,

$$T = \frac{R}{v}. \quad (2)$$

But, as shown above, the resistance is equal to the density D, hence results the equation,

$$T = \frac{D}{v}. \quad (3)$$

From the principles of "analytical mechanics" we know that the relative mean motion n of any body, great or small, moving around a "center of attraction," in obedience to the "law of gravitation," is expressed by the equation,

$$n = \sqrt{\frac{k}{a^3}}, \quad (4)$$

in which a represents the mean distance of the body. Since the above equation holds good for any value of a, we may regard the body or the particle as moving in a *circular* orbit whose mean distance, a, is equal to r, or the radius of the spherical mass to which the particle belongs; and as k is equal to the mass M, equation (4) may be written $n = \sqrt{\frac{M}{r^3}}$ or, since in a circular orbit $n = v$, we may write the following: $v = \sqrt{\frac{M}{r^3}}$, substituting this value of v in equation (3) the following results,

$$T = \frac{D}{\sqrt{\frac{M}{r^3}}}. \quad (5)$$

Since the density of any spherical body is proportional *directly* to the mass, and *inversely* to the cube of the radius of the sphere, the *relative* density will be given by the equation, $D = \frac{M}{r^3}$. From this we see that the denominator in the second member of equation (5) is equal to $\sqrt{D}$; therefore that equation becomes,

$$T = \frac{D}{\sqrt{D}} = \sqrt{D}, \quad (6)$$

i. e., the "relative time of rotation" is equal to the square root of the "relative density," which was to be proven.

The existence of the law having been thus demonstrated,

there arises the question, to what cause or causes can the discrepancies noted in column 3, be justly attributed?

Four possible causes can be enumerated:

First, possible error in the determination of the "time of rotation" and of the mass and radius of the body, the two last, of course, involving the density.

Second, the retarding action of an accumulation of fluid matter analogous to a "tide wave," raised upon the body by the attractive influence of neighboring bodies.

Third, the non-coincidence of the centre of gravity and that of figure.

All of these causes may operate in the case of any cosmic body, so that the discrepancies shown in the above tabulation can exist without casting any doubt whatever upon the legitimacy of the hypothesis above advanced.

Since the rotation periods, the masses, and the radii of the principal members of the "solar system" are known quite accurately, the first named cause can be properly charged with only a very small part of the discrepancies. To one or both of the second and third causes the latter are, therefore, mainly due. An example of the operation of the second cause can be found in the case of the moon; the density of our satellite is approximately 0.61 that of the earth; therefore, according to my hypothesis, the *relative* time of the lunar rotation should be equal to the square root of this relative density, or to 0.78; but it is known from observation that the time of the moon's axial rotation is nearly the same as that of the orbital revolution, or nearly twenty-seven days, instead of about three-fourths of a day, the determination from the density. A part, or even the whole of this difference can be attributed to the operation of a "tide wave" generated in the originally fluid matter of the moon by the attractive force of the earth, this wave acting as a *break* opposing the rotational movement and reducing the "time of rotation" to nearly equality with that of the orbital revolution of our satellite. It is evident that this retarding influence must depend upon the mass and the distance of the neighboring body, and upon its time of revolution relative to the rotation period of the retarded body, and it is also obvious that the maximum effect from the second cause will have been produced when the time of rota-

tion has been made thereby equal to the time of orbital revolution. As the earth has acted against the lunar rotation, so must the moon have affected the earth through the originally fluid mass of the latter. Even now the friction of the oceanic "tide wave" is recognized as a cause operating to retard the "diurnal motion." The effect of the action of the moon upon the earth must, of course, have been very much less than that produced by the action of the earth upon its satellite, because of the relative smallness of the lunar mass. Planets having satellites are particularly subject to this retarding influence, but all are more or less affected by the action of the sun and of other members of the system. The third cause may be effective in the case of any planet or satellite, and may operate even against the sun's rotation.

There is a fourth and very important cause which may act to modify the relation between the density and the "time of rotation," but it is one that operates, probably, only in the case of the sun. The sun's relative density is known to be 0.25, the square root of which, or 0.5, should, according to the hypothesis, represent the relative solar rotation period; but it is well known that the sun rotates upon its axis once in 25.3 days, instead of once in twelve hours; in other words, its actual "time of rotation" is nearly fifty-one times as great as that determined theoretically from the density. It may seem that this great discrepancy between the fact and the theory would render the latter untenable, but, in reality, such is not the case; the hypothesis as stated above is founded upon the condition that the only force in action is the centripetal force of gravity due to the mass of the body under consideration.

From preceding equations the following are easily deducible:

$$T = \frac{D}{\sqrt{\frac{k}{r^3}}} = \frac{D}{\sqrt{\frac{M}{r^3}}} = \sqrt{D}.$$

Now it is obvious that since the values of M, r, and therefore of D, are quite accurately known, the only quantity whose value is in doubt is k, or the attractive force. It is true that when gravity is unopposed, k is equal to M, and the relations expressed by the above equation hold good; but if there be in action in the solar matter a force directly

opposed to gravitation, but whose influence does not extend to the bodies of the "solar system" from whose movements the value of M is determined, k will not be equal to M, and although the values of the mass and of the radius, and, consequently, of the density, be accurately known, they will not give the true relative "time of rotation" through equation (4). The difference between the value of T determined from the density, and that derived from observation, will be a measure of the force acting against solar gravity.

If we consider the equation,

$$T = \frac{D}{\sqrt{\frac{k}{r^3}}}$$

we can readily see that if we should use that value of k which is equal to M, and which is determined from the motion of the earth, the resulting value of T would be 0.5, or one-half a day; but, as above stated, observation shows that its real value is 25.3 days, or 50.6 times as great as the theoretical determination. To reduce them to equality it is necessary that the value of k should become only the $\frac{1}{2560}$ of its value due to the mass. Of course this determination of the value of k may be too large because T may be affected by one or all of the three causes already mentioned, but as the influence of these is probably comparatively small, it may be permissible for the purposes of illustration, to assume that the whole difference between the "time of rotation" determined theoretically from the density, and that revealed by observation, is due to a force operating in the solar matter directly against gravity and reducing the latter to the $\frac{1}{2560}$ part of the value that it would otherwise have.

This hypothetical antagonistic force may be either the expansive action due to the tremendous thermic energy of the sun, or a repulsion resulting from the action of the correlated force, electricity, or, as is very probable, the result may be brought about by the operation of both of these possible causes. I think that observation furnishes evidence of the existence of such a force. It is known that "terrestrial attraction" is a force that will cause a body near the surface of the earth to fall towards the centre, and to attain a velocity of about thirty-two feet per second, this velocity which is, in the case of the earth, denoted by g, being

equal to k; if the body be not free to fall, the restraint will give rise to the pressure called weight, and in the case of the sun, the value of k or M, determined from the motion of the earth, is such as to make the surface gravitation about twenty-seven times as great as "terrestial gravity," *i.e.*, it is a force that will cause a body, in falling, to attain a velocity of 872 feet per second, and to weigh twenty-seven times what it would upon the surface of our globe. A volume of matter that would weigh one pound here, would, on the sun, weigh twenty-seven pounds; but if the attractive force, represented by k, be reduced by the operation of the antagonistic force to the $\frac{1}{2560}$ part of the value due to the mass, it follows that the above mentioned volume of matter would, if on the sun and free to fall, attain a velocity of only 0.34 feet per second, and would weigh only $\frac{27}{2560}$ of a pound, instead of twenty-seven pounds. As before stated, this value of the opposing influence may be too great, and it is used simply for the purpose of illustration; but it is evident that the existence of any *considerable* force, so acting, would remove a great difficulty in the way of a conception of certain phenomena transpiring upon the sun's surface, and which are, otherwise, very puzzling. I refer to the astonishingly great degree of mobility with which the surface matter of the sun seems to be endowed, and which is evinced by the opening and the closing of spots or cavities covering millions of square miles of the sun's surface, and the upheaval of vast quantities of solar matter, to the height of several hundred thousand miles, in a space of time so short that a force capable of producing the observed effects is, judging from terrestrial analogy, almost inconceivable.

Such action upon the sun suggests the possibility of the existence of a force acting against gravity among the particles of the earth's atmosphere. Living, as we do, at the bottom of the atmospheric ocean, it is impossible that we should have much *definite* knowledge of the condition and properties of the upper portion of the gaseous envelope which surrounds the earth, for the greatest elevation ever attained or attainable by man, and even the highest stratum capable of sustaining a cloud, lie far below the upper limit; yet we know that the atmosphere is a mechanical mixture

of gases, and that a distinctive property of a gas is *expansive* force. Such a force operating in the surface matter of the sun or in the earth's atmosphere, would render both more sensitive to the disturbing action of neighboring bodies than they would otherwise be, and thus the possibility of the agency of these bodies in the production of sun spots and atmospheric disturbances, as claimed in my paper on "The Effects of Rotation," published in Nos. 69, 70 and 72 of the SIDEREAL MESSENGER," is the more easily conceivable.

The relation between the density and rotation is intimately connected with the conditions of equilibrium, and in a fluid envelope such as the atmosphere, whose density decreases with the height, these conditions may be such that the influence of outside bodies may produce a very considerable disturbance. Finally, the existence of the relation between density and rotation just discussed, furnishes, I think, strong proof in favor of the truth of the "nebular hypothesis," which regards the members of the "solar system" as formed from originally independent particles of matter falling toward and moving around a common centre in obedience to the law of gravitation; for it is only by regarding them as so formed that the equation connecting the "relative time of rotation" with the "relative density" can be deduced.

CURRENT INTERESTING CELESTIAL PHENOMENA.

THE PLANETS.

Mercury sets later than the sun during the whole of May, and will be in very favorable position for observation in the evening from the 15th to the 30th. It will be at greatest elongation, east 22°49′ from the sun, May 24; in conjunction with the moon, 1°53′ north, May 31, 10 A. M.; stationary in right ascension June 6.

Venus has just passed inferior conjunction with the sun, 4° north of the latter, April 30, and cannot therefore be seen without telescopic aid. At the end of the month the planet will again be visible in the east before sunrise. During the past two months we have had many exquisite views of the "evening star," and have obtained several very good pho-

tographs, but have been unable to detect any markings upon its surface with the eight-inch equatorial. Venus will be stationary in right ascension May 19, and at greatest brilliancy June 5.

Mars is beyond the sun and sets at nearly the same time as the latter, so that no observations of it can be made.

Jupiter may be observed after midnight and will soon be in position for evening observation. He is in the constellation of Sagittarius in a bright portion of the Milky Way. Jupiter will be in conjunction with the moon May 17 at 9 P. M. Central time; to observers in Africa the planet will be occulted by the moon, but in our northern latitude it will pass near the northern edge of the moon. June 14 at 3 A. M. the planet will again suffer occultation by the moon, visible between the equator and about 40° of north latitude. Several observers have reported successful observations of the occultation of March 24.

Saturn will be visible in the west in the early evening, but at so low an altitude that observations of the satellites will be unsatisfactory. We therefore omit the ephemeris of the satellites. In March Dr. Terby of Louvain, France, announced the appearance of a white region upon the rings of Saturn next to the shadow of the planet. Many observers have examined the planet under favorable circumstances since that time and while some have reported the white region as distinctly seen and always in the same position, most have seen nothing more than a slight brightening of the rings, where they seem to be cut off by the shadow, and which appeared to be simply the effect of *contrast* with the intense blackness of the shadow. The *Astronomical Journal*, No. 191, contains a report by Professor Holden of the observations made at the Lick Observatory with both the 36-inch and the 12-inch refractors. On the best nights the observers were able to detect nothing abnormal, but when the definition was poor an ill-defined, yellowish "lump" could be seen "extending across rings A and B about half as wide as the shadow, and close to it." "A similar appearance was seen across both A and B where they met the ball of the planet on the s. p. side." On March 21, at the suggestion of Mr. Schaeberle, the experiment was tried of placing an occulting bar in the eyepiece and bringing it over

different parts of the planet. Wherever the bar was placed a brighter confused lump seemed to rise and to border the bar. If the the bar was over the ball, this brighter border extended all across the disc. If it was placed on the ring the appearance was just that of the lump bordering the true shadow of the ball on rings. The observers, therefore, concluded that there was nothing abnormal upon the rings, but that the appearances seen were due to bad atmospheric conditions alone. A perusal of Dr. Terby's letter to the editor of the *Astronomische Nachrichten* (A. N. No. 2887 p. 109) will probably lead one to the same conclusion with regard to his observations.

Uranus is in very favorable position for evening observation. He may be easily found with an ordinary telescope, being in field of a finder with θ Virginis, the nearest naked eye star northwest of Spica. No reports of observations of this planet this year have reached us as yet.

Neptune is too nearly in line with the sun to be seen.

MERCURY.

	R. A.	Decl.	Rises.	Transits.	Sets.
	h m	° ′	h m	h m	h m
May 20	5 25.2	+25 30	5 35 A.M.	1 30.6 P.M.	9 26 P.M.
25	5 48.6	+25 17	5 38 "	1 34.2 "	9 29 "
30	6 04.4	+24 31	5 40 "	1 30.4 "	9 20 "
June 5	6 12.6	+23 07	5 32 "	1 15.0 "	8 58 "
10	6 10.3	+21 45	5 17 "	12 53.1 "	8 29 "
15	6 01.5	+20 25	4 55 "	12 24.5 "	7 54 "

VENUS.

May 25	2 03.3	+12 00	2 58 A.M.	9 49.7 A.M.	4 41 P.M.
June 5	2 18.3	+11 38	2 31 "	9 21.4 "	4 12 "
15	2 41.7	+12 38	2 11 "	9 05.3 "	4 00 "

MARS.

May 25	4 37.6	+22 36	4 43 A.M.	12 23.6 P.M.	8 04 P.M.
June 5	5 10.1	+23 34	4 27 "	12 12.8 "	7 58 "
15	5 39.8	+24 03	4 15 "	12 03.1 "	7 51 "

JUPITER.

May 25	18 29.5	−23 07	9 48 P.M.	2 13.2 A.M.	6 38 A.M.
June 5	18 24.6	−23 08	9 00 "	1 25.0 "	5 50 "
15	18 19.4	−23 12	8 16 "	12 40.5 "	5 05 "

SATURN.

May 25	9 10.9	+17 28	9 41 A.M.	4 56.1 P.M.	12 12 A.M.
June 5	9 14.2	+17 12	9 02 "	4 16.2 "	11 31 P.M.
15	9 17.8	+16 56	8 27 "	3 40.3 "	10 54 "

URANUS.

May 25	13 08.3	− 6 34	3 15 P.M.	8 52.8 P.M.	2 30 A.M.
June 5	13 07.5	− 6 29	2 31 "	8 08.7 "	1 46 "
15	13 07.0	− 6 27	1 51 "	7 28.9 "	1 07 "

NEPTUNE.

	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
May 25	4 01.4	+19 01	4 32 A.M.	11 54.8 A.M.	7 18 P.M.
June 5	4 03.1	+19 06	3 43 "	11 05.9 "	6 29 "
15	4 04.6	+19 10	3 04 "	10 27.9 "	5 52 "

THE SUN.

	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
May 20	3 50.6	+20 08	4 27 A.M.	11 56.3 A.M.	7 25 P.M.
25	4 10.8	+21 04	4 23 "	11 56.7 "	7 30 "
30	4 31.1	+21 53	4 20 "	11 57.3 "	7 35 "
June 5	4 55.7	+22 38	4 17 "	11 58.3 "	7 40 "
10	5 16.4	+23 04	4 15 "	11 59.3 "	7 43 "
15	5 37.1	+23 21	4 15 "	12 00.3 P.M.	7 46 "

Occultations Visible at Washington.

Date.	Star's Name.	Magnitude.	IMMERSION. Wash. Mean T. h m	IMMERSION. Angle f'm N. P't. °	EMERSION. Wash. Mean T. h m	EMERSION. Angle f'm N. P't. °	Duration. h m
May 30	ζ Tauri.	3½	7 10	97	8 06	263	0 56
31	μ Geminorum.	3	4 23	16	4 42	350	0 19
June 1	δ Geminorum.*	3½	7 20	132	8 13	250	0 54
13	Jupiter.		16 59	356	Jupiter 0.2′ N of moon's limb.		

Phases of the Moon.

		Central Time. d h m
Last Quarter	May	21 3 53 P.M.
New Moon	May	29 11 20 A.M.
First Quarter	June	6 2 02 P.M.
Full Moon	June	13 7 58 A.M.

Phenomena of Jupiter's Satellites.

Central Time. d h m		Central Time. d h m	
May 17 9 43 P. M.	III Sh. In.	May 29 9 54 P. M.	II Sh. In.
18 12 31 A. M.	III Sh. Eg.	11 07 "	II Tr. In.
1 04 "	III Tr. In.	30 12 33 A. M.	II Sh. Eg.
3 22 "	I Ec. Dis.	30 1 46 "	II Tr. Eg.
3 57 "	III Tr. Eg.	June 3 1 39 "	I Ec. Dis.
19 12 29 "	I Sh. In.	10 45 P. M.	I Sh. In.
1 19 "	I Tr. In.	11 15 "	I Tr. In.
2 45 "	I Sh. Eg.	4 1 02 A. M.	I Sh. Eg.
3 35 "	I Tr. Eg.	1 31 "	I Tr. Eg.
9 50 P. M.	I Ec. Dis.	10 50 P. M.	I Oc. Re.
20 12 53 A. M.	I Oc. Re.	5 12 36 A. M.	III Oc. Re.
10 02 P. M.	I Tr. Eg.	6 12 30 "	II Sh. In.
21 12 21 A. M.	II Ec. Dis.	1 24 "	II Tr. In.
22 9 57 P. M.	II Sh. Eg.	7 10 11 P. M.	II Oc. Re.
22 11 26 "	II Tr. Eg.	11 12 39 A. M.	I Sh. In.
25 1 41 A. M.	III Sh. In.	12 59 "	I Tr. In.
4 30 "	III Sh. Eg.	10 01 P. M.	I Ec. Dis.
4 30 "	III Tr. In.	11 52 "	III Ec. Dis.
26 2 23 "	I Sh. In.	12 12 34 A. M.	I Oc. Re.
3 04 "	I Tr. In.	3 54 "	III Oc. Re.
11 45 P. M.	I Ec. Dis.	9 03 P. M	IV Ec. Dis.
27 2 39 A. M.	I Oc. Re.	9 24 "	I Sh. Eg.
9 30 P. M.	I Tr. In.	9 36 "	IV Ec. Re.
11 08 "	I Sh. Eg.	9 41 "	I Tr. Eg.
11 47 "	I Tr. Eg.	14 9 21 "	II Ec. Dis.
28 2 55 A. M.	II Ec. Dis.	15 12 25 A. M.	II Oc. Re.

* A Multiple Star.

Approximate Times of Transit of the Great Red Spot Across the Middle of Jupiter's Disk.

Central Time.	d	h	m	Central Time.	d	h	m	Central Time.	h	d	m
May	15	11	38 P. M.	May	27	3	39 A. M.	June	6	1	53 A. M.
	17	5	25 A. M.		27	11	30 P. M.		6	9	44 P. M.
	18	1	16 "		29	5	17 A. M.		8	3	31 A. M.
	18	9	07 P. M.		30	1	08 "		8	11	22 P. M.
	20	2	54 A. M.		30	8	59 P. M.		10	5	09 A. M.
	20	10	45 P. M.	June	1	2	46 A. M.		11	1	00 "
	22	4	32 A. M.		1	10	37 P. M.		11	8	51 P. M.
	23	12	23 "		3	4	24 A. M.		13	2	38 A. M.
	23	8	14 P. M.		4	12	15 "		13	10	29 P. M.
	25	2	01 A. M.		4	8	06 P. M.		15	4	16 A. M.
	25	9	52 P. M.								

Occultations of the Planet Jupiter as Observed at the Lick Observatory March 23, 1889.

[*Communicated by Edward S. Holden.*]

I beg to forward with this the observations of the occultation of Jupiter made at the Lick Observatory March 23. The altitude of the planet was too small to allow the great equatorial to be used. Mr. Keeler observed with the 6½-inch Clark equatorial. Mr. Barnard observed with the 12-inch Clark equatorial. Mr. Hill observed with the 4-inch Clark comet-seeker. He also computed the times of immersion and emersion. Mr. Leuschner, Cand. Phil., observed with a portable Clark telescope of 3 inches aperture.

Their results follow:

OBSERVATIONS BY MR. KEELER.

My observations were made with a low power on the 6½-inch equatorial. At immersion Jupiter was very low, and the definition was so bad that no detail could be seen on the surface of the planet. No attempt was made to note the time of any occurrence.

The limb of the moon at the point of contact was much brighter than Jupiter. I estimated the point of equality of brightness to be about 5 diameters of the planet, or 3′ from the limb.

At emersion the definition was better, but still so bad that the belts on Jupiter could only occasionally be seen. The only curious appearances noticed were those due to irradiation. The satellites, which were large and brilliant, appeared suddenly, leaving a feeling of surprise that such (apparently) large discs could be so quickly uncovered by the

motion of the moon. The disk of the planet when it emerged appeared to be notched into the dark limb of the moon, which was easily visible. The observation of other phenomena than these was prevented by the bad seeing.

J. E. KEELER.

OBSERVATIONS BY MR. BARNARD.

The immersion of the planet and satellites was observed under very unfavorable conditions, the planet being low and the air excessively disturbed. The satellites were very faint at contact, the unsteadiness of the moon's limb preventing any accurate observation of their disappearance. I think the observations of disappearance will scarcely be out over one second, however.

Recorded Times of Disappearance.

		h	m	s	
Satellite	III	14	3	45.1	disappeared in undulations—very faint.
"	II	14	6	46.6	" " " " "
"	I	14	9	37.2	probably 1s early; disappeared as above.
Jupiter	I	14	11	31.9	images very unsteady; Contact I will be more
"	II	14	12	41.7	accurate, the disturbance being greater at II.

Satellite IV, though pretty distinct before reaching the limb, was completely blotted out some four seconds before contact by an undulation of the limb that did not cease for ten seconds after the satellite had disappeared.

At reappearance the seeing was somewhat slightly improved, though still unusually bad.

The approximate times and angles of reappearance having been projected by Mr. Hill, the reappearances were caught sharply. The satellites were bright and very large in coming out—seeing = 1 or 2 on a scale of 5. They shot out rather rapidly, though a sensible interval was required —from 0.2*s* to 0.3*s*. As near as could be judged, the times of the reappearances of the centers of the satellites were recorded.

The reappearance of Limb I of Jupiter was that of a very ill-defined mass of boiling light, which grew rapidly, without any special form; as it progressed the limb of the moon was seen cutting the disc, but so badly distorted that nothing could be made of it. At last contact the planet had scarcely any definite form, and the belts were hardly recognizable.

Recorded Times of Reappearance.

		h	m	s			h	m	s
Satellite	III	15	11	53.6	Jupiter	I	15	19	58.8
"	II	15	14	47.8	"	II	15	21	21.2
"	I	15	18	40.8	Satellite	IV	15	35	27.5

From the bad definition, nothing worthy of note was seen. The observations were made with full aperture of the 12-inch equatorial, a power of 150 being used. The times are Mt. Hamilton mean time. E. E. BARNARD.

OBSERVATIONS BY MR. HILL.

The observations were made with the 4-inch broken tube comet-seeker, on the roof of the main building; times noted on M. T. chronometer Negus 1719. The prism of this instrument gives poor images of bright stars and planets, and the moon was very low in the heavens at each phase of the phenomenon. In addition, the atmospheric disturbance was greater than I have before seen it at this Observatory. For these reasons the only records marked "good," at time, were those of "first glimpse" of planet and satellites, at the emersion. It was entirely impossible to make any notes of physical appearances, etc.

Immersion observed with power of about 25 diameters, not sufficient to distinguish between light of planet and that of moon. Made two guesses at contact and disappearance, as below; chronometer correction being applied, to reduce to Mt. Hamilton M. T.:

	h	m	s	
Contact I =	14	11	36.2	:
" II =	14	12	26.7	::

Emersion: For this used a somewhat higher power from the 6-inch equatorial, giving with this instrument, about 60 diameters.

	h	m	s	
Reappearance Satellite III =	15	11	54.0	Good.
" " II =	15	14	48.1	"
" " I =	15	18	41.2	"
" Jupiter (III) =	15	19	58.9	"
" " (IV) =	15	21	18.8	Poor.
" Satellite IV =	15	35	29.6	Good.

Could with difficulty distinguish dark limb of moon at this observation. CHAS. B. HILL.

OBSERVATIONS OF MR. LEUSCHNER.

The occultations were observed with a portable telescope of 3¼-inch aperture and a terrestrial eyepiece magnifying

about 40 diameters. The times were taken from sidereal chronometer No. 1720. The satellites preceding Jupiter were fairly well seen until about 3–5*s* before disappearance when they were lost in the glare. The moon's limb was very unsteady and seeing exceedingly poor. Jupiter touched the moon's limb several times before making a complete contact and the time was not recorded for this reason. The second contact occurred at 14*h* 12*m* 42.9*s* L. O. M. T. The disappearance of the last satellite could not be observed, as it was lost about 4*s* before disappearance. Mr. Keeler, however, observed it near by in the 6-inch equatorial and called time, which I recorded.

Disappearance of 4th Satellite, 14*h* 25*m* 52.2*s* L. O. M. T. M. Keeler estimates that this time may be 2*s* out. The reappearances were all observed except the third contact of Jupiter. The times are as follows:

		h	m	s			h	m	s
Satellite	I	15	11	53.8	Jupiter	IV	15	21	14.8
"	II		14	48.3	Satellite	IV		35	28.5
"	III		18	42.2					

The Polar Rays of the Corona. In speaking of this point, in a recent private letter, Professor W. H. Pickering remarked that the difference in the character of the rays at the two poles of the sun, noticed in 1886, was well marked at the last eclipse. Some of the long jets, seen by the naked eye in 1886, were carefully searched for at the last eclipse and not found. These did appear on the photographic plates of this year.

The outline of the moon was followed for nearly four minutes after totality by the aid of a 4½-inch glass. Numerous photographs of the corona were taken by the Harvard party after the third contact.

Speaking of the spectroscopic observations Professor Pickering also remarked that the spectrum was much simpler than in the eclipse of 1882, showing perhaps a half dozen lines besides the hydrogen. In the spectra of the protuberances the H and K lines were strongly marked. The hydrogen rings which did not come out with the prismatic camera in 1886, were moderately well shown. During totality forty-seven negatives in all were secured, and about twenty within a few seconds afterwards which can be used with advantage.

Ephemeris of Comet e 1888 (*Barnard, Sept. 7*). The following is a continuation of my ephemeris of Barnard's comet from Berberich's elements, for the month of April:

G. M. T.	R. A. h	m	s	Decl. °	′	Log r	Log Δ
April 1.5	23	34	23	−0	28.8	0.2941	0.4608
3.5		34	4	−0	20.0	0.2962	0.4598
5.5		33	43	−0	11.3	0.2984	0.4586
7.5		33	19	−0	2.7	0.3007	0.4572
9.5		32	54	+0	5.9	0.3030	0.4555
11.5		32	26	+0	14.3	0.3053	0.4536
13.5		31	56	+0	22.6	0.3077	0.4516
15.5		31	23	+0	30.9	0.3101	0.4493
17.5		30	46	+0	39.0	0.3126	0.4468
19.5		30	7	+0	46.9	0.3150	0.4442
21.5		29	24	+0	54.7	0.3176	0.4413
23.5		28	38	+1	2.4	0.3201	0.4382
25.5		27	48	+1	10.0	0.3226	0.4349
27.5		26	54	+1	17.3	0.3252	0.4314
29.5		25	55	+1	24.4	0.3278	0.4278

O. C. WENDELL.

Harvard College Observatory, March 22, 1889.

[The above ephemeris was received too late for last month's MESSENGER, but is published by request of certain astronomers, who desire to compare their observations with the places given in the same.—ED.]

The Orbit of Sappho (*80*). Robert Bryant, F. R. A. S., has done astronomy great service in a recent thorough and apparently complete discussion of the orbit of the minor planet Sappho (80). The particular object in the mind of the author in doing this laborious work at the present time is that the orbit of this planet may be well known, so that it may be observed at opposition, with the heliometer, for the study of the sun's parallax. Dr. Gill thinks that minor planets Victoria, Sappho and Ariadne are the best situated of any for this kind of work.

Photographing the Great Nebula of Orion. In the March number of the *Monthly Notices* Isaac Roberts gives a brief paper on what he terms photographic analyses of the Great Nebula of Orion, M 42 and M 43 and *h* 1180 in Orion. This was done by exposing negatives between 5 seconds of time and 205 minutes, and studying the gradations of the nebulosity obtained, in order "to compare the relative actinic power of the light in different parts of the nebula." The first exposure of 5 seconds showed the four stars of the

trapezium. The second exposure of 30 seconds increased the diameter and density of those stars, and a third exposure of 1 minute intensified the same effect and showed the beginning of nebulosity around θ. The fifth exposure of 6 minutes made the star images one with irregular outline, and brought out the nebulosity more fully, giving points for comparison with the well known drawings of Rosse. When the exposures were continued for 15 minutes or more these resemblances more or less disappeared on account of the increasing density of the nebulosity. The interesting question whether this nebula is in a state of change is, at least, presumptively answered in the affirmative by Mr. Roberts' series of photographs. On this point he says: "It is obvious, if we compare the positions of the stars within the nebula, as they are shown on the charts by Lord Rosse and by Bond, that changes in the relative position of some of them have taken place since the year 1866, and I shall here just refer to one as an illustration. In the Trapezium the two stars numbered 65 and 69 on Lord Rosse's chart are shown much closer to each other than the stars numbered 67 and 73, and Bond's chart agrees with Lord Rosse's, whereas in the photographs these pairs are nearly equidistant."

Within the last few months Mr. Roberts has made exposures on this nebula varying from 30 minutes to 205 minutes. Those made during 30 and 81 minutes show great extensions of the nebula, and M 43 and M 42 are joined, "*h* 1180 is well developed with characteristic dark cross streams." An exposure of 205 minutes, Feb. 4, 1889, gave evidence that all three of these objects belong to one and the same gigantic nebula. The inferences which Mr. Roberts seems to draw fairly from his work up to the present time are intensely interesting, indeed, almost startling. We give his concluding words:

"The evidence and confirmation place it beyond reasonable doubt that the links shown between these objects are realities, and though they supply us with vastly extended knowledge of the dimensions and form and details of this nebula, yet leave us with unsatisfied desire to see more, and probably to find that the nebula will be shown to have a symmetrical form in external outline, but is in a state of strong

internal commotion. Next year we ought to be able to supply the missing links, and see it as a finished picture. In the mean time we ought, with all gratitude, to admire the patient, long-suffering endurance of those martyrs to science who, during the freezing nights of many successive winters, plotted, with pencil in benumbed fingers, the crude outlines which have been handed down to us as correct drawings of this wonderful nebula, which we can now depict during four hours of clear sky with far greater accuracy than is possible by the best hand-work in a lifetime."

EDITORIAL NOTES.

A new comet was discovered by E. E. Barnard, astronomer at Lick Observatory, March 31.7215 G. M. T. in apparent right ascension 5*h* 20*m* 49.3*s*, in apparent polar distance 73°53′, having daily motion in right ascension, — 13′ of arc; in polar distance, — 2′. The comet at time of discovery was on the southern border of the constellation Taurus, about 12° east of Aldebaran; its motion during last month was slowly to the south-east. April 4, observations of the comet were made by E. Lamp, Weiss, and C. F. Pechüle. It was then very faint—too faint for the use of the micrometer by the first named observer.

J. M. Schaeberle, astronomer at Lick Observatory has computed the following

ELEMENTS:

$$T = 1889 \text{ May } 26.44 \text{ G. M. T.}$$

$$\left.\begin{aligned} \pi &= 286^\circ\ 3' \\ \Omega &= 297\ \ 26 \\ i &= \ 90\ \ \ 0 \end{aligned}\right\} \text{Mean Eq. } 1889.0$$

$\log q = 8.6064 = 0.0404$ for q, which appears to be very small for the perihelion distance. The following places for April are by Dr. H. Oppenheim, as given in A. N. No. 2889.

	R. A.	Decl.	L.
	h m	° ′	
April 4	5 17.9	+15 39	1.10
8	15.1	50	
12	12.7	41	
16	5 10.8	+15 32	1.62

By the above table it is apparent that the light of the comet is increasing, but its position for observation is not very favorable, as it is only 2½ hours east of the sun.

Photographic Study of Stellar Spectra at Harvard College Observatory. The third annual report from the Harvard College Observatory, on work which constitutes the Henry Draper Memorial, under the direction of Professor E. C. Pickering, is received. The plan of previous years has been continued during the last. The five special lines of research are: 1. Catalogue of spectra of bright stars. Instrument, 8-inch Batche telescope, a photographic doublet, as an objective. Work: photographs cover the entire sky north of —25° declination, exposures from 5 to 10 minutes. About 28,000 spectra of 10,800 stars of 7th magnitude and brighter have been examined. Copy of catalogue is nearly ready for the printer, as far as 14*h* in right ascension, the positions being brought forward to the epoch of 1900. 2. Catalogue of spectra of faint stars. Instrument used is the 13-inch photographic telescope. Work of taking photographs required to cover the sky north of the equator was nearly finished in November, 1888. Since that time the instrument has been in use in the Western and Southern expeditions.

3. Detailed study of the spectra of bright stars. The instrument for this work is the 11-inch refractor with one, two or four large prisms over its objective. With this instrument 686 photographs have been taken, most of them with an exposure of two hours. Photographic plates now in use are sensitive enough to get 570 stars north of —30° declination with one prism, 170 with two prisms and 87 with four prisms. It is expected that this work will be completed during the next year.

4. Faint stellar spectra by the aid of the 28-inch reflecting telescope by Dr. Draper has not been continued.

5. Catalogue of spectra of bright southern stars is now in progress by the 8-inch Batche telescope in Peru. The sky from—25° to the south pole will be covered, and the resulting photographs sent to Cambridge and reduced as in the case of the northern stars. The work will probably require two years.

6. The catalogue of spectra of faint southern stars will be extended to the south pole simultaneously with the observations just referred to above, which are being made in Peru. These memorial funds are being put to excellent service in the interest of astronomy, as is plainly seen from this important report.

Occultation of Jupiter. The occultation of Jupiter by the moon March 24, 1889, took place here at sunrise. The sky was very clear but there was so much light that the disappearance of the satellites could be seen with difficulty, and at reappearance they had faded entirely out of sight. The disappearance of satellite III was noted at March 23d 17*h* 45*m* 0.5*s* but with some uncertainty as to the exact instant.

The times of disappearance and reappearance of the planet were noted at the following instants, Ann Arbor mean time:

		h	m	s
Dis.	First contact	17	55	18.2
"	Last "	17	57	39.9
Reap.	First "	18	47	14.7
	Last "	18	48	55.4

The disappearance was on the bright edge, 30° to 40° from the north point. The reappearance of the planet's edge outside the moon's limb was not seen at the instant it occurred. The time given is several seconds late.

The 6-inch equatorial was used with an eye-piece magnifying about 200 diameters, careful watch was kept for the remarkable contact phenomena which have been sometimes described for occultations of planets, but nothing noteworthy was observed. There was some tremulousness and flickering at contact, but nothing else. M. W. HARRINGTON.

Ann Arbor, Mich.

Comet 1882 II. We have received the first part of an "Investigation of the Comet System 1843 I, 1880 I and 1882 II,"* by Dr. Heinrich Kreutz, which is indeed a model work of its kind. It was published in 1888 as a Publication of the Observatory in Kiel, Germany. The author gives first a short but complete review of the apparition of Comet 1882 II (the great September comet), followed by an ephemeris depending upon Dr. Stechert's elements of the orbit. He then discusses briefly the passage of the comet over the disc of the sun, showing that it entered upon the western edge of the disc Sept. 17, 4*h* 30.6*m* Berlin mean time, passed off the eastern edge at 5*h* 47*m*, reached perihelion at 6*h* 24*m* at a distance of less than half the radius of the sun from his

* Untersuchungen uber das cometensystem 1843 I, 1880 I and 1882 II. I Theil Der grosse September comet 1882 II. Von Dr. Heinrich Kreutz, zweitem Observator der konigl. Stenwarte. Kiel, 1888.

east limb, passed behind the east edge of the sun at 7*h* 59*m*, and out from behind the west edge at 9*h* 58*m*. The first of these times was observed by Finlay and Elkin at the Cape of Good Hope, the comet totally disappearing as it passed upon the solar disc, so that the observers thought that it had gone behind the sun. The comet was on the east side of the sun for 2*h* 11*m*, during which time it should have been visible in America, but appears to have been nowhere noticed.

The author then gives a list of the comparison stars, with all the observed places of each, and a complete collection of all the published observations of the comet. A discussion of the observations of the nucleus is given next. Considerable difficulty was found in identifying the parts of the nucleus which the different observers took as the points of measurement. At the time of discovery of the comet, Sept. 8, the nucleus was round, 10″-15″ in diameter, and it retained the circular form as it approached the sun. Sept. 17, half an hour before it entered upon the disk of the sun the nucleus had a diameter of only about 4″; the next day it was the same, as observed on the meridian at the Cape of Good Hope. Sept. 21.0 the nucleus was first noticed to be oval; Sept. 22.2, according to Schaeberle's measures, the major axis was 11.9″, the minor axis 4.8″. Toward the end of the month the elongation was generally noticed; Sept. 30.7 Finlay first discovered two balls of light in the head of the comet. Later other separate points of light were seen, so that the nucleus appeared to be divided into 3, 4 or 5 parts. Still later these became enveloped in a dense haze so that only one or two could be distinguished, and it is somewhat uncertain which points were the brightest toward the last. Dr. Kreutz thinks, however, that he has succeeded in nearly all cases in identifying the point which was taken for measurement and has deduced the necessary corrections to reduce each to the point which he has assumed as the center of gravity of the comet. From all the observations he has derived nineteen normal places of the comet, and after correcting these for the perturbations by Jupiter and Saturn, has formed nineteen normal equations from which are deduced the corrections to the assumed elements. The final elements thus derived are as follows:

Epoch of occultation 1882, Sept. 20.5 Berlin mean time.

$$
\begin{aligned}
T &= 1882 \text{ Sept. } 17.2612428 \pm 0.0000319 \\
\omega &= \;\;69^\circ 35' 20.80'' \pm 7.57'' \\
\Omega &= 346\;\; 00\;\; 42.70 \;\;\pm 7.31 \\
i &= 141\;\; 59\;\; 44.63 \;\;\pm 1.79
\end{aligned}
\left.\vphantom{\begin{aligned}a\\a\\a\\a\end{aligned}}\right\} \text{Mean equinox 1882.0}
$$

$$
\begin{aligned}
\log q &= 7.8893666 \pm 0.0000364 \\
\log \varepsilon &= 9.9999600 \pm 0.0000001 \\
\varepsilon &= 0.9999078 \pm 0.0000002 \\
\log a &= 1.9551 \qquad a = 84.16 \pm 0.22 \\
Period &= 772.0 \pm 2.9 \text{ years.}
\end{aligned}
$$

The nineteen normal places are represented within the errors of observation.

Dr. Kreutz discusses the possible identity of this comet with that of 1106, which was recorded as a very brilliant one, seen in daylight close to the sun. He reaches a negative conclusion from the fact that the comet of 1106 was seen north of the equator in February, which could not be true of one moving in the orbit of the 1882 comet.

The Satellites of Uranus. In the last number of your valuable journal appears my reference to a glimpse of the satellites of Uranus with a 3-inch telescope. As the statement will undoubtedly be questioned, since they are said to be "beyond the range of glasses as large as 8 or 10 inches in aperture," permit me to call attention to "Webb's Celestial Objects," fourth edition, revised, 1881. Page 186, he says: "Ward has glimpsed the two outer ones, Oberon and Titania, with a $4\frac{3}{10}$-inch Wray achromatic."

In June of last year Uranus was in splendid position for observation, and for three or four evenings in the early part of the month the seeing was very fine. At that time with Uranus in the field, I glimpsed those extremely faint little moons of the planet with my 3-inch glass. S. S. CHEVERS.

Brighton, N. J., March 19, 1889.

The above in substance is what appeared in the March MESSENGER, and attracted considerable attention, as might have been expected in view of the small aperture employed. As Mr. George A. Hill of Washington says, the satellites of Uranus are difficult objects for glasses of 8 or 10 inches aperture. "Herschel pronounced these objects the most difficult of all in the solar heavens, and gives as an example of their faintness a double star between β^1 and β^2 Capricorni.

The object glass, he says, that is not capable of splitting this double, which is about 3″ apart would not have a ghost of a chance in seeing the satellites of Uranus." Speaking of these satellites Newcomb remarks (Popular Astronomy, p. 365): "They may fairly be regarded as the most difficult known objects in the planetary system; indeed, it is only with a few of the most powerful telescopes in existence that they have been certainly seen." Professor Newcomb doubtless had in mind all four of the satellites of Uranus when the above statement was written. Lamont of Munich saw the second and fourth satellites in 1837 with a 10½-inch refractor. Professor Young says (General Astronomy, p. 368): "Titania, the largest and brightest of the satellites of Uranus, has a distance of 280,000 miles, with a period of 8*d* 17*h*. Under favorable circumstances this satellite can just be seen with a telescope of 8 or 9 inches aperture." These views are cited to show the prevailing idea of skillful observers relative to the difficulty experienced in observing these minute satellites. It is not the purpose of this note to throw doubt on the observation of Mr. Chevers. Nobody knows as well as he, probably, what he saw, especially if he observed carefully, and was certain of what he saw. On the other hand it is true, if he saw those satellites with a 3-inch telescope, that his is the finest record of observing in this particular known to modern astronomy, so far as our knowledge goes. We may fairly say that a good eye, a fine glass and exquisite seeing in Utah air at high altitude were doubtless favoring circumstances in this observational problem. Mr. Chevers will doubtless try his glass on the double star between β^1 and β^2 Capricorni that further tests of its defining power may be known; and yet, in fairness it should be added that if his glass fails to divide that double, it would not prove that he did not see what he claims to have seen.

Errors in Elementary Astronomical Text-Books. In the last number of THE SIDEREAL MESSENGER, Dr. Lewis Swift, of Warner Observatory, Rochester, N. Y., in an article on "Errors in Astronomical Text Books," gives one or two from Lockyer's Astronomy. There is one not mentioned by Dr. Swift which gave the writer of this communication a good

deal of trouble at one time, and it is therefore desirable to notice it.

In speaking of the stationary appearance of the superior planets, Mr. Lockyer says (Art. 374, p. 208): "The planet, as seen from the earth, will appear at rest, as we are advancing for a short time straight to it." As the earth is always moving in the direction of a tangent to the point of its orbit then occupied by the earth, and the tangent makes a right angle with the radius, or line between earth and sun, a stationary planet ought, if Lockyer's explanation is correct, to be 90° from the sun on the celestial sphere. In the case of the planet Mars, it is plain, without any measurement at all that Mars, when stationary, is more than 90° from the sun. The true explanation of the stationary phenomenon, given by Herschel and other astronomers, is that all lines drawn through the centers of the earth and a planet during its stationary periods are parallels, and parallels extending from any point on the earth's orbit towards the celestial sphere seem to converge to a point upon that sphere. Since a stationary planet is on such parallels it is always seen against the point to which they seem to converge, and therefore seems to us to be motionless.

It is a strong illustration of the way in which elementary astronomy is divorced from observation that Lockyer's book has been taught for nearly twenty years, but no teacher has discovered this error, which is apparent as soon as observation is used.

The same correction applies with slight changes to the subject of inferior planets as treated by Lockyer. B. A.

White Region on Saturn's Ring. This phenomenon is still visible, and has been distinctly seen on every occasion that the planet has been observed. It is visible with all powers from 80 to 450 on the 10⅛-inch equatorial. With a magnifying power of 150 it is most conspicuous. To me "the region" has appeared somewhat smaller of late, as if the shadow of the ball was encroaching upon the white spot.

I do not think the contrast theory will explain this white spot, for an observation made a few nights ago through large thin clouds proved that the white region was *intrinsically brighter* than the rest of the ring. I regard the obser-

vation as a fortunate and valuable one. These large drifting clouds acted like a graduated photometric wedge of colored glass, gradually obscuring the planet and as gradually allowing it to reappear. In every case the white spot was the last to disappear at the planet's obscuration, and the first to reappear as the planet became visible.

WILLIAM R. BROOKS.

Smith Observatory, Geneva, N. Y.
April 19, 1889.

The Harvard Party Photographs of January Eclipse. We have had the pleasure of seeing two of the photographs of the Harvard College Eclipse party who observed at Norman, California, Jan. 1, 1889. These pictures were sent us not long ago by Professor W. H. Pickering, from Los Angeles. The smaller picture was taken with a telescope of 5 inches aperture and 91 inches focus, and shows the black disk of moon seven-eights of an inch in diameter. The polar filaments are distinctly seen and the four lateral streamers are fairly well marked.

The larger picture was received later and is the finest photograph of the solar corona we ever saw. This picture was obtained by the aid of the 13-inch photographic telescope, and shows the diameter of the moon's disk to be 1⅞ inches, with streamers traceable to a distance somewhat more than the moon's diameter. The plate used was a Carbutt A, grade of sensitiveness 12, and was exposed 10 seconds, between the 38th and 48th seconds after second contact. Those beautiful polar rays must stand out on the original negative with charming delicacy, because they are so well shown in this picture. We congratulate Professor Pickering on the success of his photographic work of the late eclipse.

Observatory at Iowa College, Grinnell. A recent letter from Professor Buck, by whose earnest labors an Observatory has been recently equipped for Iowa College, informs us that a time service for the city of Grinnell is already in operation. He has contrived an ingenious device by which the steam whistles in the city may be blown daily that the Observatory time may be known. This is now done by a push-button in Professor Buck's study.

Occultation of Jupiter. The occultation of Jupiter by the moon March 23, was observed here with the 10⅛-inch equatorial. The sky was very clear, and the images fairly steady, considering the low altitude of the objects. The first contact was noted by me at 18*h* 45*m* 47*s* local sidereal time (corrected for clock error). The last contact at emersion was recorded at 19*h* 37*m* 31*s*.

My young daughter, Anna Caroline (now eleven years of age, and whom I trust I may be allowed to say manifests a promising ability for precise observation), observed the phenomena with the 3-inch finder of the large instrument, and independently recorded the first contact one second earlier than above. After geometrical contact at immersion the moon's limb *seemed* to me to become indented, to give way, so to speak, before the planet, so that the limb of Jupiter appeared visible nearly three seconds after true geometrical contact. Although observed in bright sunshine, Jupiter appeared very distinct and the belts were plainly visible. Magnifying powers used were 80 diameters by myself on the 10⅛-inch; and 30 by Anna on the 3-inch finder.

WILLIAM R. BROOKS.

Smith Observatory, Geneva, N. Y.
March 25, 1889.

Astronomical Society Proposed. The *Globe-Democrat* of St. Louis recently contained the following item from Mr. F. H. Burgess:

Your suggestion in a recent issue, that wealthy St. Louisans should endow an astronomical Observatory here, deserved more attention than it seems to have received. The excellent work done by the St. Louis party in the eclipse expedition shows that we have astronomical ability here of the highest order, only it needs encouragement. I suggest first the formation of an Astronomical Society to awaken an interest in the subject, and I should be glad to receive the addresses of any who, like myself, are students of the "sublime science," and would like to come together and see what can be done in the line of your excellent suggestion.

The Great Telescope for Los Angeles, Cal. The following note was taken from the *Alta California* of Feb. 28, 1889:

The great telescope which it is proposed to put upon Wilson's Peak, near Los Angeles, will perhaps not be built. Alvan Clark, who was invited there by the trustees of the University of Southern California to look over the ground and consider the practicability of the scheme, made a favorable report and named a price of less than $200,000 to erect a 40-inch lens instrument and necessary accessories. But that offer was not accepted. There is not much more than $200,000 available to spend upon the entire Observatory, and the purchase of the telescope is not more than half the expense. Mr. Clark left Los Angeles without an order, and the scheme will probably never be carried out.

The Chamberlin Observatory. The building for this Observatory will be begun in a few weeks; it is to be situated six miles from the heart of Denver, on a plat of ground containing two blocks. It will be constructed of lava stone; the length is 65 feet, and the extreme depth 45 feet. A steel dome, 37 feet in external diameter, and having its summit 45 feet above the ground, will surmount it. The chief rooms are the library, computing room, director's office, instrument room, transit room, clock room, dome room with the observer's room adjoining it, photographic dark room and janitor's room.

The disks for the 20-inch object-glass are almost ready in the optical works of Mantois. The Clarks will grind the lens. Fauth & Co. of Washington have the contract for the large equatorial mounting, and have begun its construction. It will be the first new instrument to be equipped with Saegmuller's finding circles, at the eye-end. This firm will also furnish the Observatory with a six-inch equatorial mounting (glass by Brashear), a three-inch meridian circle, two standard clocks, a chronograph, two micrometers, and several other subsidiary instruments.

The crown lens of the large object glass will be reversible, for photography. The Observatory will be a department of the University of Denver, to which Mr. H. B. Chamberlin, its founder, will present it.

The White Spot on Saturn. On the evening of March 26, Professor H. L. Smith, of Hobart College, Geneva, N. Y., examined the white spot on Saturn with his Spencer telescope of 4¾ inches aperture, 35½ inches focal length, with powers from 100 to 350. His report is that he cannot make up his mind that the phenomenon is anything more than the effect of contrast of the shadow and the ring, yet it seems a little too conspicuous to be explained in that way only.

March 24th another very skillful observer tried the white spot on Saturn and was unable to see anything unusual. By the aid of the spectroscope this observer could see G. F. b E. D. and C? (a strong line in the red) with perfect certainty, and with glimpses of a multitude of lines more in the yellow, green and blue, but could not find any traces of bright lines.

Notch in the Terminator of Venus. William Edward Wood of U. S. Architect's office, Washington, D. C., while observing Venus March 13, with 3½-inch refractor, with various eye-pieces, including a Steinheil achromatic, believes he saw a notch in the terminator about one-fifth of the way from the north horn inward toward the south horn. Special care seems to have been given to the observation to assure its faithfulness and the distinctness of the notch remained. Subsequent observations of the terminator by several astronomers with good instruments fail to show the notch described; but this might happen on account of the change of the terminator. The observation is mentioned that astronomers generally may consult their note books for further information at or near the date March 13.

Astronomical Clock Record. Frequently from time to time, we have requests for information concerning the performance of good astronomical clocks, by persons with and without experience in accurate time-keeping. As an example of good work in this direction we append the record of an astronomical clock owned by Mr. Chas. H. Rockwell, Tarrytown, N. Y., for a brief period during last year. The clock is the Pond Motor, Gerry escapement, with a Rockwell pendulum.

1888.		Correction.	Daily rate.	1888.		Correction.	Daily rate.
		s	s			s	s
August	24	−2.289		Setpember	21	2.573	+0.012
	26	2.353	+0.032		24	2.603	0.011
	30	2.497	0.036	October.	4	2.033	−0.057
September	5	2.700	0.034		8	2.178	+0.036
	13	2.471	−0.029	(Obliged to stop the clock.)			

Persons desiring further information will find Mr. Rockwell always an interested and helpful correspondent.

Double Star Observations by S. W. Burnham, of Lick Observatory. A paper containing the observations of double stars, new and old, made by S. W. Burnham, astronomer at Lick Observatory, during August, September and October of 1888 has been received. The new double stars were discovered and measured by the 12-inch or 36-inch equatorial belonging to the Observatory. The paper is entirely devoted to objects of special interest in this branch of astronomy.

New Instruments for the Chamberlin Observatory. In connection with our note on the late work of Messrs. Fauth & Co., Washington, D. C., in last month's issue, we inadver-

tently omitted to mention that the company had been awarded the contract for furnishing a large part of the instruments of the new Chamberlin Observatory of the University of Denver, Colorado. Messrs. Fauth & Co. are to make the mounting for the 20-inch equatorial, to furnish a 3-inch transit circle, chronograph, sextant with artificial horizon, mean time clock, mounting for 6-inch equatorial, a solar transit level and a spherometer. These instruments will be finished in the best style of workmanship and some of them will have improvements recently devised by the company that are new, convenient and apparently very useful.

Death of William Tempel. We were pained to learn, some days ago, by kindness of Charles W. Dunn of Florence, Italy, of the death of Professor William Tempel, Director of the Arcetri Observatory. This sad event took place on the 16th of February, after a long and painful illness. A brief sketch of the life and work of Professor Tempel will be given in a later number of this journal.

BOOK NOTICES.

A Treatise on Trigonometry, by Professors Oliver, Wait and Jones of Cornell University. Ithaca, N. Y.: Dudley F. Finch, Publisher, 1889.

The first edition of this book was published in 1881. Its plan was first outlined by Professor Jones, and submitted to the other professors of the University whose names are given above. After full discussion of its plan, the first edition was published as the joint production of all. During the last eight years this text has been used in Cornell University, going through one revision in the meantime. We have now before us a second revision which bears the date of 1889 on its title page. From a comparison of this with the first edition, we find so many and so important changes that it may be fairly said to be wholly rewritten, and that virtually the book is a new one. In the earlier part of it, the matter is made more simple and direct, and vastly improved in our judgment for use in the class-room. Further on, we find new matter in the way of applications of trigonometry to surveying, astronomy and navigation, which will serve a good purpose for those students who have the time to follow up the branch in this way, and to learn something of the vast range of mathematical physics herein sug-

gested. The discussion of the general triangle, plane and spherical, is an interesting feature in this book, and one that the college student ought to know something of, before he passes on to other branches of mathematics in regular course, if he shall at all have right notions of trigonometry as a science, or realize its uses as an instrument of mathematical investigation. The attention of teachers of mathematics is called to this text-book on trigonometry.

Logarithmic Tables, by Professor George William Jones of Cornell University. Published by Dudley F. Finch, Ithaca, N. Y., 1889. Flexible covers, pp. 72.

This appears to be an excellent book of tables. Its arrangement is certainly first rate. The table of logarithms of numbers is six place, and for four figure numbers, with right hand column on each page for differences. The page is open, and the figures, with variety of face, are easy for the eye. The table of constants in natural and logarithmic numbers is unusually full, and gives latest values, so far as we know. The arrangement of the trigonometric tables so as to place natural and logarithmic numbers side by side, is unusual, but these tables present this so neatly and well as to give convenience without confusion. To promote the detection of errors in these tables Professor Jones offers one dollar for the first notice of each error.

The Student's Atlas in twelve Circular Maps. On a Uniform Projection and one scale. With two Index Maps, intended as a *Vade-Mecum* for the Student of History, Travel, Geography, Geology, and Political Economy. By Richard A. Proctor. Publishers, Messrs. Longmans, Green & Co., London. Also New York, 15 East 16th Street. 1889.

This atlas was prepared by Mr. Proctor, in the course of his studies, to meet his own wants as a student, in the desire to form exact ideas of the relations of different parts of the earth's surface to each other. The one scale in all the maps is a new feature and deserves attention because not in general use by those who have been esteemed the best makers of atlases for geography. The way Mr. Proctor came to adopt the one scale for all his terrestrial maps was partly accidental. In preparing projections of the earth for his "Seasons Illustrated" the "Old and New Astronomy" and "Studies of the Transits of Venus," he found the one-scale plan much more convenient and satisfactory than that

in use in ordinary atlases, and so it was adopted, and the terrestrial maps needed for his numerous publications were so made. The maps of this atlas are reductions from those prepared for earlier works. They are circular and have a diameter of nearly seven inches, are printed in colors, show prominent physical features, names of prominent places in clear type, and have on the back of each map its contents as a convenient reference title. The index maps are also an interesting feature, showing at a glance how the six maps belonging respectively to the northern and southern hemispheres are related to one another. As a whole we do not know of an atlas so convenient and handy in form as this, and therefore commend it to the attention of students generally.

Lessons in Elementary Mechanics by W. H. Grieve, P. S. A. Publishers, Messrs. Longmans, Green & Co., London. Also, New York, 15 East 16th Street.

We have before us two small books which were prepared by Professor Grieve to meet the need of the separate stages of instruction in mechanics or elementary natural philosophy, as defined by what is known as the schedule of the New Code in use in the various schools under the school board of the city of London.

The book designed for the third stage has for its subject matter the simple mechanical powers, liquid pressure, the hydrostatic press, liquids under the action of gravity, the parallelogram of forces and velocities, with numerous examples under each topic. The language is simple and direct, and the illustrations and cuts well chosen.

The second book for the second stage is more difficult and deals with matter in motion, the weight of a body, its inertia and momentum, measures of force, work and energy, energy may be transformed, but can not be destroyed, and heat as a form of energy.

The methods employed in tracing these themes are essentially the same as in the other book. No mathematics above the common arithmetic is needed to perform the examples. The review questions at the close of each chapter are an excellent feature of this stage, not only on account of points raised in them, but also others which are easily suggested by them.

STAR ATLAS.

Containing Maps of all the Stars from 1 to 6.5 magnitude, between the North Pole and 34° South Declination, and of all Nebulæ and Star Clusters in the same region which are visible in telescopes of moderate powers. With explanatory text by

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